Interactive comment on "Floe-size distributions in laboratory ice broken by waves" *by* Agnieszka Herman et al.

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This manuscript concerns laboratory experiments in which lab-grown ice is broken by water surface waves, and the shape and type of ice pieces formed due to the fracture process. The laboratory experiments are clearly articulated and the rationale is quite clear. The authors do a good job of discussing an important area of sea ice physics that will benefit greatly from such experimental work. I have concerns about the fitting procedure and analysis that should be cleaned up in review, but otherwise find this manuscript to be important work worthy of publication. Comments are below.

Introduction.

Page 1 Line 23 - Please cite some of the "growing evidence that the FSD... " etc as

this is not supported in the text.

Page 2 Line 10 - Here some discussion of Virkar and Clauset (2012), and subsequent papers would be useful.

Page 2 Line 17 - I would be more careful to avoid being subject to the same criticisms you levy earlier: "obtained Gaussian pdfs" (the claim) and "fit observations to a Gaussian" (the reality) are different things. Same with the later comment on the FSDs produced using DESIgn.

Experimental Design

I'm a little unclear as to the applicability of these test conditions to the real world. The scale of fractured ice and surface waves in the model experiment are quite small relative to the scale of floes even in the Southern Ocean. Obviously there are experimental constraints but a discussion of how this 72x10x2.5/5 m tank relates to a strip of ice on the sea ice margin would be helpful. In addition, where might one expect the thermodynamic conditions?

Page 3 Line 17 - If I read this correctly, there are really only two tests being performed rather than many groups of tests, as one sheet of ice is broken. Does one expect path-independence of the FSD? If not this is a shortcoming that should be discussed.

Page 3 Line 30 - What about salinity/salt in the water?

Page 4 Line 23 - How do these attenuation rates relate to those used in popular attenuation parameterizations?

Page 5 Line 1 - Again, how does this relate to sea ice conditions in the "real world"?

Page 5 - Image processing - Can you be more specific about the image processing methodology? For example, to produce a binary image one might employ a thresholding value, but the results may be very sensitive to this parameter. How substantial were the "manual corrections", and how sensitive were the final results to the image process

parameters?

Page 6 Line 5 - I think one should re-define "surface area" as "basal surface area", or simply "area" here, as much of the interest in the FSD has focused on the "lateral surface area" component of floes, which seems to impact lateral melting (i.e. Steele, 1992, Roach et al, 2017).

Page 6 Line 10 - Some discussion of what the "number-weighted FSD" is would be helpful, as it is not clear from this how the distributions discussed here are related to the other "FSD"s that proliferate in the literature.

Page 7 Line 6 - If interest is in the FSD, why should we are about the range of order of magnitudes of surface area, why not report this in terms of the distribution of effective radii, or at least b_f?

Page 8 Line 1 - The entire discussion on fitting is somewhat difficult to parse given the authors (correct) insistence on mathematical and scientific scrutiny of the power-law hypothesis. The 5 adjustable parameters have little connection to physics, and the concept of what "meaningful values" of the tunable parameters might be is unclear. While the authors do some testing of the coefficients, they don't do any real hypothesis testing. A way of doing this is to draw random distributions from the model, and compute a p-value based on the fraction of those random distributions are closer to the model than the observed distribution (via a K-S statistics, for example).

This is not the only hypothesis worth testing: another is that any data would pass this test. One could perform the same test, but replace the observed distribution with a power-law or gaussian. Given the 5 adjustable parameters, I think this needs to be done.

Page 9 - Discussion

How does one explain the relatively rectangular character of the ice here, relative to the relatively circular character of the ice in real conditions? Is the grinding of floes a

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significant factor in this?

I would like to see a plot of something like mean floe size in time (or as a function of breaking event #), as this is of importance for models of the FSD.

The discussion of the sum-of-two-distributions idea is well-taken, but a discussion of other processes that act on floes other than those influenced by waves would be good to have, in particular the fact that these processes may or may not be dominant regionally or hemispherically (i.e. for example, if in the Southern Ocean, waves are important but not in the Arctic).

The authors are free to contact me with any questions.